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**The Case of α_s : Z versus Low Energies
or
How Nature Prompts us of New Physics**

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Abstract

The values of α_s determined from low- and high-energy measurements are in irreconcilable contradiction with each other. The current status of the problem is critically reviewed. Consequences of the α_s contradiction, in conjunction with other anomalies detected at the Z peak, are discussed. The write-up is updated in accordance with experimental numbers reported at summer conferences.

1 Introduction

About a year ago I was at the Glasgow Conference. Quite a few talks there were devoted to the global fits at the Z peak. The message of all of the speakers, one after another, was the same: everything perfectly fits the Standard Model (SM), no indications of new physics are detected. The conclusion was so astonishing and so obviously wrong, that shortly after I wrote a “sociological” paper, whose message was perfectly opposite [1]. If one takes the Z data on α_s seriously, one *must* insist that new physics is already with us. The value of the strong coupling constant, $\alpha_s(M_Z) \approx 0.125$, as it is usually quoted in connection with the measurements at the Z peak, is too high to be compatible with low-energy phenomenology. It presents a clear signal of the presence of something beyond the Standard Model. This “something” may very well be low-energy supersymmetry, with sparticle masses in the 100 GeV ballpark, or a more exotic beast.

Today, the psychological climate is totally different. The contradiction between the low-energy and high energy determinations of α_s is perceived seriously by many. In several publications that have appeared recently [2] – [5] this contradiction serves as the starting point for dedicated analyses showing that the quality of global fits at the Z peak improves if, instead of the Standard Model, one considers its supersymmetric generalization, MSSM. More than that. Discussion of implications of low α_s for the general model-building started [6] – [11]. One of the ideas is getting information about the GUT scale corrections, including the masses of the superheavy particles, which results, in turn, in new and quite specific predictions concerning the proton lifetime [6]. Another idea is relaxing various mass relations routinely imposed on MSSM for aesthetical reasons [10, 11]. One then asks a down-to-earth question: “what values of the sparticle masses could explain the observed excess in $\alpha_s(M_Z)$?” The answer turns out to be exciting. Let me quote, for instance, Wells and Kane [11]: “... stop and chargino must be light enough to be detected when the energy of LEP is increased to over 140 GeV ... If a stop or chargino is not found then either R_b excess will go away, or if it persists, the SUSY explanation is not relevant...” Here, the authors mention also, another puzzle of the Z physics, R_b , to be discuss in brief later on. The so called R_b crisis which surfaced recently tends to eclipse the confrontation between the low- and high-energy values of α_s . It should be stressed, however, that these two problems are totally unrelated to each other experimentally since the methods used for measuring R_b and α_s at the Z peak are completely different. Even if one assumes, for a short while, that the R_b excess is a spurious instrumental effect, this need not be the case with α_s . At the very least, one should say that there are two independent measurements signaling deficiencies of the Standard model at the Z peak, so that the probabilities that these two effects are statistical fluctuations are multiplied. Thus, it is impossible to overestimate the lead provided by $\alpha_s(M_Z)$, especially in conjunction with the R_b crisis, the only new hints we’ve gotten from Nature over a decade.

My talk consists of three parts. First, I will argue that the genuine strong

coupling constant at Z is close to 0.11. Second, I will briefly discuss implications of this fact for physics at the 100 GeV energy scale. At the end I will comment on this problem in the context of Grand Unification.

2 Large versus small $\alpha_s(M_Z)$

When I say small or large α_s , I mean the following: if $\alpha_s(M_Z)$ is close to 0.11 it will be referred to as small; if it is close to 0.125 it is large (α_s is defined in the $\overline{\text{MS}}$ scheme [12]). The distinction between these two values is clearly seen on Fig. 1 which presents a compilation of data on different measurements of α_s , essentially borrowed from Bethke's talk [13], with a few points updated. First, I erased a couple of points with error bars too large to be informative. They merely overshadow the general picture. Added is a point obtained recently by Voloshin [14] from analysis of the QCD sum rules in the $b\bar{b}$ channel. The result claims to have error bars so small, they are barely seen on the plot. I hasten to add though, that the estimate of the uncertainties presented in Ref. [14] seems to refer to the uncertainty of a particular procedure, and should be taken with caution. Finally, the lattice prediction for α_s is quoted from the recent talk [15].

The pattern is quite obvious: all low-energy measurements cluster around a small value of the strong coupling constant, $\alpha_s(M_Z) \approx 0.11$, with one notable exception of τ decays, to be discussed later. Determinations of α_s that are the cleanest from the theoretical standpoint are those done in the Euclidean domain – deep inelastic scattering, lattices, and the Voloshin sum rule. The corresponding four points are marked by the arrows.

At the same time, the conventional routine of determining α_s at the Z peak under the SM assumptions leads to high values, clustering around 0.125.

The τ decays will be subject to special scrutiny below, and you will hopefully see that the theoretical uncertainty usually quoted is grossly underestimated. If a proper value of this uncertainty is used, the τ point must be merely ignored as uninformative. The drawback of the τ analysis is its essentially Minkowskian nature. Nonperturbative contributions in the Minkowski domain are expected to be essentially larger (and die off much slower) than similar contributions “from the other side”, in the Euclidean domain. Moreover, it is very difficult (if possible at all) to control them in the Minkowski domain based on the truncated condensate series.

Taken at its face value, the discrepancy between the small and large α_s clusters might seem quite marginal – this is a two σ effect or so, and who cares about 2σ effects? Being expressed in terms of the low-energy parameters the difference becomes pronounced, however. Indeed, the large value of $\alpha_s(M_Z) = 0.125$ is translated in $\Lambda_{\overline{\text{MS}}}^{(4)} \approx 480$ MeV, to be compared with $\Lambda_{\overline{\text{MS}}}^{(4)} \approx 200$ MeV appearing in the small- α_s case. Although routinely used, $\Lambda_{\overline{\text{MS}}}^{(4)}$ is far from being a perfect parametrization. (As a matter of fact, the $\overline{\text{MS}}$ scheme as a whole is rather unphysical, but this is a subject

for another story. It is commonly used, by convention, and I will follow this convention too for the time being.) At the very least, it makes more sense to compare $\Lambda_{\overline{\text{MS}}}^{(3)}$ – this parameter is more relevant to the low-energy hadronic phenomenology and is somewhat larger than $\Lambda_{\overline{\text{MS}}}^{(4)}$. Anyway, the situation is quite transparent. From the point of view of the low-energy QCD the contradiction between the first and the second values of $\Lambda_{\overline{\text{MS}}}^{(4)}$ is qualitative and irreconcilable. If the scale parameter of QCD is so high one must be prepared to say farewell to a whole wealth of results accumulated in QCD over years. The success of QCD sum rules in dozens of problems referring to all aspects of hadronic phenomenology must be considered as a pure coincidence then, a conclusion which seems quite fantastic.

To illustrate my point let me show you a typical plot one deals with in the analysis of the sum rules. (Fig. 2). The plot presents the Borel-transformed correlation function Π of two vector currents (with the isotopic spin $I = 1$), $J_\mu = (1/2)(\bar{u}\gamma_\mu u - \bar{d}\gamma_\mu d)$, versus M , the (Euclidean) Borel parameter. Figure 2 shows the integral

$$I(M) = \frac{2}{3M^2} \int ds R^{I=1}(s) e^{-s/M^2}. \quad (1)$$

The imaginary part of this correlation function in the Minkowski domain is known, and is proportional to the total cross section of the e^+e^- annihilation into hadrons with the isotopic spin I equal to 1. This cross section is measured with a high precision near the ρ meson and below. The accuracy gets somewhat worse at energies above 1.5 GeV, but this fact is irrelevant for two reasons: (i) at $M < 1.1$ GeV the ρ meson contribution exceeds 70%; (ii) it is the absolute normalization of the individual measurements of $R^{I=1}(s)$ which is most uncertain. This means that the genuine experimental curve runs inside the corridor labeled by “U” and “L” on Fig. 2; the shape of the curve is “parallel” to the upper and lower sides of the corridor (“U” and “L”). In other words one can slightly shift the curve for $I(M)$ upwards or downwards, as a whole, without distorting the shape of the curve. It is practically impossible to deform the *form* of the curve since it results from integration over a large number of points.

The experimental corridor I use was obtained in Ref. [16] in the following way. Each individual point in the set of all measurements of $R^{I=1}(s)$ has a systematic uncertainty. If the uncertainties in *all* individual points are taken with the plus sign we get the upper side (“U”), if they are taken with the minus sign we get the lower side (“L”). It is clear that a more careful analysis could significantly narrow the corridor, the more so that several new points obtained since 1979 have smaller error bars. But even the old data seem to be sufficient to make a definite conclusion.

The most remarkable thing is not what we see on the plot but, rather, what we do not see there. Namely, if we *descend* from higher values of M to lower ones, within the window, the experimental curve shows no indication whatsoever of growth or explosion of $I(M)$. On the contrary, $I(M)$ stays practically constant (although this constant value is somewhat uncertain), and then smoothly goes down at $M \sim 1.0$ GeV, i.e. slightly above the ρ meson mass. If we believe that the description based

on perturbation theory plus a few nonperturbative condensates is applicable in the window shown on Fig. 2 we have to conclude that α_s is small. The curved labeled by “220” on Fig. 2 corresponds to $\Lambda_{\overline{\text{MS}}}^{(3)} = 220$ MeV and the standard values of the gluon and four-quark condensates. The shaded area indicates my estimate of the theoretical uncertainty due to α_s^3 terms and higher condensates (the uncertainty in the gluon and four-quark condensate is not included). It is clearly seen that this theoretical curve is reasonable. At the same time, if $\Lambda_{\overline{\text{MS}}}^{(3)}$ is in the vicinity of 550 MeV (this value follows from $\alpha_s(M_Z) = 0.125$) the strong coupling constant calculated as a function of M explodes at ~ 1.5 GeV. As a reflection of this explosion the theoretical curve goes up sharply, as shown on Fig. 2. Even $\Lambda_{\overline{\text{MS}}}^{(3)} = 400$ MeV is hardly acceptable. To give an idea of the role of the condensates I plot two curves corresponding to $\Lambda_{\overline{\text{MS}}}^{(3)} = 400$ MeV – one with the standard value of the four-quark condensate, and the other one (labeled by “2 QC”) with twice the standard value.

The difference is striking and qualitative. If $\Lambda_{\overline{\text{MS}}}^{(3)} < 250$ MeV the explosion occurs far below the window, so that no reflection of the explosion is seen in the theoretical curve. The pattern of behavior in the transitional domain is decided mostly by the condensate corrections. For large values of $\Lambda_{\overline{\text{MS}}}^{(3)}$ we are unable to descend to the domain where the condensate corrections play a role. The explosion of the perturbative series occurs earlier.

I emphasize that this situation is typical for the QCD sum rules. That is why I concluded that they can not be reconciled with the high value of α_s . Needless to say this is not a mathematical theorem. Being stubborn, one could always insist that the flatness of $I(M)$ could be a result of a general conspiracy – $\Lambda_{\overline{\text{MS}}}^{(3)}$ is very large, terms of all orders in α_s are large at $M \sim 1$ GeV but they combine to produce a smooth curve. All successful predictions of sum rules are mere coincidence. As usual, the theory of general conspiracy is impossible to rule out by rational arguments.

2.1 The Z peak kitchen.

At least four experimental groups have extracted α_s , by doing different measurements at Z (see e.g. [17, 18]). The most precise approach, both theoretical and experimental, is the measurement of the total hadronic width of the Z . Not only is this method most straightforward experimentally, the theoretical uncertainty here is the smallest since (i) this is the only quantity calculated to the order $\mathcal{O}(\alpha_s^3)$; (ii) non-perturbative corrections can be argued to be minimal in the total width. In the $\overline{\text{MS}}$ renormalization scheme, and assuming the validity of the Standard Model, the result for the total hadronic Z width Γ_h has the form

$$\Gamma_h = \Gamma_0 \left(1 + r_1 \frac{\alpha_s(M_Z)}{\pi} + r_2 \frac{\alpha_s(M_Z)^2}{\pi^2} + r_3 \frac{\alpha_s(M_Z)^3}{\pi^3} \right) \quad (2)$$

where Γ_0 is the parton model width (with the electroweak corrections included), and the coefficients r_1, r_2 and r_3 are obtained by computing relevant Feynman graphs, see [19, 20] and references therein. Nice reviews are also given in Refs. [21, 22].

The coefficients depend on the quark masses; the most noticeable uncertainty of this type is due to the errors in the t quark mass. The coefficient r_1 is unity (modulo tiny mass corrections), r_2 is of order unity, r_3 is negative and is of order 10. This pattern suggests that at order α_s^4 the asymptotic nature of the α_s series will, perhaps, show up so that including the $\mathcal{O}(\alpha_s^4)$ term is not going to improve the theoretical accuracy [23]. At the moment the experimental uncertainty in this particular quantity, Γ_h , dominates so that there is no point in discussing other sources of uncertainty. Comparing Eq. (2) with

$$\Gamma_h^{exp} = 1744.8 \pm 3.0 \text{ MeV}$$

one arrives at

$$\alpha_s(M_Z) = 0.125 \pm 0.005, \quad (3)$$

see Ref. [18]. This number assumes that there are no contributions beyond the Standard Model.

As I have already mentioned, there are good reasons to believe that nonperturbative effects in Γ_h are negligible since the energy release is sufficiently high.

Notice that even the leading (first) order term in α_s presents a small correction ($\sim 4\%$). A 13% shift in α_s amounts to 5 permille effect in Γ_h . In absolute numbers this is of order 10 MeV.

Here we come to a remarkable observation: the experimentally measured yield in the $b\bar{b}$ channel is higher than the SM expectations by 11 ± 3 MeV (the R_b crisis). Is this a mere coincidence? It is tempting to say no. Assuming, that these extra 11 MeV in the $b\bar{b}$ width are due to new physics and subtracting them from Γ_h we get [18]

$$\alpha_s(M_Z) = 0.102 \pm 0.008,$$

in perfect agreement with the low-energy determinations. This fact was mentioned already in 1993 by Altarelli *et al.* [24] but was largely ignored and forgotten. The Glasgow Conference rapporteurs mentioned in passing R_b as the only little light cloud on the face of the Standard Model, hastily adding that the signal is statistically insignificant [25]. Now, of course, people are more inclined to speculate on the impact of the R_b excess. The situation remains fogged, however, since, in addition to the R_b excess, there is a hint in the data on the deficit in the $c\bar{c}$ yield, so that the excess in the $b\bar{b}$ yield seems to be offset. Moreover, this shortage finds no natural explanation in the minimal supersymmetric extension of the Standard Model [2, 3, 11, 18] casting shadow on all data referring to the heavy quark yields at Z . The experimental uncertainty in R_c is larger than that in R_b , though, which prevents unambiguous conclusions at present. Once again, I suggest to use the α_s information to make a *prediction*: when the dust is settled an overall excess in Γ_h in the ballpark of 10 MeV will be confirmed.

2.2 Survey of low-energy determinations of α_s

Many compilations of the low-energy determination of α_s share a common drawback: they are plagued by “no discrimination policy”. Those who compile the estimates forget (or do not pay attention) that QCD is a peculiar theory whose infrared behavior remains unsolved, and various approaches enjoy different degree of control over the large distance dynamics. On the other hand, all experimentally measurable quantities do involve infrared dynamics. Even those which are claimed to be infrared stable are typically protected from the large distance contributions only in perturbation theory. Therefore, not all low-energy evaluations α_s are equal to each other. In particular, I will discard, from the very beginning, all analyses of the jet parameters (thrust and so on). Theoretical formulae one uses to extract α_s from these measurements are purely perturbative. No reliable methods were suggested so far allowing one to estimate, even roughly, non-perturbative corrections since these processes are not amenable to operator product expansion (OPE) [26]. Moreover, if in the processes tractable within OPE the nonperturbative corrections start from quadratic terms $1/Q^2$ or $1/E^2$ in the jet physics (and in the hard processes without OPE in general) linear $1/Q$ or $1/E$ nonperturbative corrections are argued to be quite abundant [27]. Attempts to build phenomenology of the $1/Q$ terms based on renormalons and related ideas are in its infancy. Even the question of their universality is being debated at the moment, to say nothing about reliable estimates of the coefficients in front of $1/Q$. Some estimates existing in the literature [27] indicate that the $1/Q$ corrections can be so large that they invalidate all existing determinations of α_s from the jet physics. The same criticism applies to the low- x physics at HERA.

Thus, we will consider only those analyses where at least some control over the nonperturbative effects exists at the present level of understanding of QCD. This leaves us with deep inelastic scattering (DIS) [28], including the Gross-Llewellyn-Smith and similar sum rules, inclusive decay rates of the type $\Upsilon \rightarrow \text{photon} + \text{hadrons}$ or $\tau \rightarrow \nu + \text{hadrons}$, QCD sum rules and, finally, lattices. Each of these approaches has its advantages and drawbacks, and we will briefly discuss them in turn. From the theoretical point of view, deep inelastic scattering and the QCD sum rules are the “cleanest” processes, at least in principle, since they allow control over each and every aspect of calculation. The inclusive widths are essentially Minkowskean quantities; as we will see shortly, this brings in an additional theoretical uncertainty which is hard to estimate. Finally, the lattice calculations are burdened by systematic errors, associated with finite size effects and, especially, putting chiral dynamical quarks on the lattice, whose understanding is not yet fully settled.

(i) Deep inelastic scattering

Data on deep inelastic scattering are very abundant. Many high statistics experiments were analyzed with the aim of extracting α_s and testing the QCD evolution.

QCD predicts not the structure function themselves but, rather, evolution with Q^2 increasing. Nonperturbative effects are represented by higher twists; in the Euclidean domain, where the analysis is carried out, practically, it is quite sufficient to limit oneself to twists two and four. To illustrate the subtle points of the analysis let us turn to a particular work [29]. At the very end, I will quote the world average for α_s from DIS.

Figure 3a, borrowed from Ref. [29], shows the Q^2 evolution of the structure function F_2 for different values of x . The experimental points are fitted according to the predictions of QCD, in the next-to-leading logarithmic approximation (NLO), i.e. at two loops. The dashed line visualizes the Q^2 evolution without the higher-twist effects. It is seen that at x lying in the interval 0.2 to 0.5 the power corrections are sufficiently small. At higher values of x the power corrections increase, on the one hand, and the quality of the data becomes worse, on the other. Lower values of x are more sensitive to the gluon distributions, which may bring in unwanted model dependence. Thus, the above interval of x is optimal. To avoid contamination from higher twists, determination of α_s has to be performed in the “high- Q^2 ” domain where the power corrections are negligible. Depending on the particular value of x chosen, this domain stretches above 5 to 10 GeV². In this domain the logarithmic derivatives $d \ln F_2 / d \ln Q^2$ are very nearly proportional to $\alpha_s(Q^2)$, with an x dependent proportionality coefficient that depends only weakly on the x dependence of the measured F_2 . Comparing the measured logarithmic derivatives with those obtained in QCD in terms of $\alpha_s(Q)$ one fits the strong coupling constant (see Fig. 3b). Neither the uncertainty in the gluon distribution nor the higher twist corrections are important provided one limits oneself to the interval $0.2 < x < 0.5$.

The theoretical uncertainties are due to the (virtual) heavy flavor thresholds and due to the scale ambiguity. The scale ambiguity emerges because we truncate the perturbative prediction at the next-to-leading order. Therefore, at this order we do not know exactly the argument of the quantities involved; it may be Q^2 or $\mu^2 = kQ^2$ where k is a number of order one. Only the next-to-next-to-leading order calculation fixes k at the next-to-leading order. Practically, the scale ambiguity is the largest theoretical uncertainty. Figure 4 shows the sensitivity of the α_s determinations to scale parameters introduced in a certain way. In Ref. [29] it is suggested to vary k between, say, one quarter and four. This would correspond to theoretical uncertainty ± 0.004 in $\alpha_s(M_Z)$. From experience accumulated in the last few years (for instance, from the BLM scale setting procedure [30]) we know, however, that it is extremely unlikely that $k > 1$. In all problems treated in the literature k turns out to be < 1 , a result perfectly natural on physical grounds since typically the momentum Q is shared between several quarks and/or gluons. If so, the theoretical uncertainty should be taken with the minus sign only. The fitted value of $\alpha_s(M_Z)$ in Ref. [29] is 0.113; experimental and theoretical errors are 0.003 and 0.004, respectively. Figure 4 demonstrates, to my mind, very convincingly, that $\alpha_s(M_Z) = 0.125$ is way beyond what is allowed by the DIS data.

More recently, the Q^2 evolution of the non-singlet structure function at high Q^2

(i.e. $Q^2 > 150 \text{ GeV}^2$) was analyzed by the CCFR Collaboration [31]. Their result is

$$\alpha_s(M_Z) = 0.111 \pm 0.002 \text{ (stat)} \pm 0.003 \text{ (syst)}.$$

The fresh world average of the α_s determinations from the neutrino and muon deep inelastic scattering is presented in Ref. [32], which gives practically the same number,

$$\alpha_s(M_Z)_{DIS} = 0.112 \pm 0.005. \quad (4)$$

(ii) The Gross-Llewellyn Smith (GLS) sum rule

This sum rule predicts the value of the integral

$$\int_0^1 F_3^{NS}(x, Q^2) dx = 3 \left\{ 1 - \frac{\alpha_s(Q)}{\pi} + \dots + 1/Q^2 \text{ corrections} \right\}. \quad (5)$$

In the asymptotic limit $Q^2 \rightarrow \infty$, the right-hand side tends to 3. If we make Q^2 sufficiently large so that the higher-twist effects are already unimportant deviations from 3 fix the value of α_s . A state-of-the-art theoretical description of the Gross-Llewellyn-Smith sum rule was presented recently [33], including the next-to-next-to leading order perturbative corrections [34] and higher twist effects [35]. This work triggered a new data analysis by the CCFR Collaboration; the experimental values for the GLS integral versus Q^2 were updated. I give here the plot (Fig. 5) borrowed from Ref. [36] (see also the review talk [32]). The solid line on this plot corresponds to a QCD fit with $\Lambda_{\overline{\text{MS}}}^{(5)} = 150 \text{ MeV}$ (or $\alpha_s(M_Z) = 0.112$). It is important that the α_s corrections are negative. Therefore, with Λ increasing the fit curve would shift further down, in clear conflict with the data plotted on Fig. 5. The outcome of the analysis is

$$\alpha_s(M_Z)_{GLS} = 0.108 \begin{smallmatrix} +0.003 \\ -0.005 \end{smallmatrix} \text{ (stat)} \pm 0.004 \text{ (syst)} \begin{smallmatrix} +0.004 \\ -0.006 \end{smallmatrix} \text{ (higher twist)}. \quad (6)$$

(iii) The QCD sum rules

In the beginning of my talk I presented a plot visualizing a typical sum rule in the classical ρ meson channel (Fig. 2). A few explanatory remarks are in order here concerning both the theoretical and experimental sides of the sum rule. For a general introduction to the method, see Ref. [37].

The right-hand side of the sum rule (1) is, in principle, measurable. In practice the cross section of the e^+e^- annihilation to hadrons with the unit total isotopic spin is rather poorly known at $E = \sqrt{s} > 2 \text{ GeV}$. If M is sufficiently small, however, the exponential weight in Eq. (1) damps the high-energy tail of the integral. Even if the integrand is known in this domain with 10% accuracy (integrally), which seems to

be more than realistic, the impact on the uncertainty of the sum rule will be at the level of a fraction of one percent provided $M \sim 1$ GeV, i.e. totally negligible for our purposes. Therefore, we can use the fact that at $E > 2$ GeV, the measured value of $R^{I=1}(s)$ must coincide with the one calculated perturbatively with the accuracy better than 10%. We then glue a theoretical tail to the experimental curve keeping in mind that the left-hand side is very insensitive to variations of the tail within the reasonable limits.

The perturbative calculation of $R^{I=1}(s)$ is carried out at three loops [38]; with three active flavors we have

$$R^{I=1}(s) = \frac{3}{2} \left\{ 1 + \frac{\alpha_s(s)}{\pi} + 1.64 \left(\frac{\alpha_s(s)}{\pi} \right)^2 - 10.2 \left(\frac{\alpha_s(s)}{\pi} \right)^3 + \dots \right\}. \quad (7)$$

In the domain of interest α_s/π is in the ballpark of 0.1. A glance at Eq. (7) leads us to conclude that the third term is of the order of the second one, and the truncation of the perturbative series is near a critical point. We will return to this issue later on. Here I note that under the circumstances, including the third term in the analysis does not improve the accuracy of the theoretical prediction. Rather, we should look at it as a natural measure of the maximal theoretical accuracy one can achieve. The relative strength of the third term is close to 1%; this means that any analysis in this energy range can measure α_s with the 10% uncertainty, at best, which translates into 3% uncertainty in $\alpha_s(M_Z)$. This is not bad, indeed, since the difference between the large and small $\alpha_s(M_Z)$ is around 13%.

With this understanding in mind we proceed to comparing the theoretical and experimental sides of the sum rule (1). Let us start from the theoretical side known since almost prehistoric times,

$$\begin{aligned} I(M) = 1 + \frac{\alpha_s(M)}{\pi} + 2.94 \left(\frac{\alpha_s(M)}{\pi} \right)^2 + \mathcal{O}((\alpha_s/\pi)^3) + \\ + C_G \langle \mathcal{O}_G \rangle \frac{1}{M^4} + C_q \langle \mathcal{O}_q \rangle \frac{1}{M^6} + \mathcal{O}(M^{-8}). \end{aligned} \quad (8)$$

Here \mathcal{O}_G and \mathcal{O}_q are the gluon and the four-quark condensates, respectively [37], and C_G and C_q are their coefficients. A few comments are in order here concerning the perturbative and nonperturbative parts represented by the first and the second lines in Eq. (8).

Notice that the α_s^2 coefficients in Eqs. (7) and (8) are different. The reason for this is the Borel transformation used to obtain the sum rule (1). It is not difficult to show that under this transformation α_s/π in R (and in the Adler D function) goes into

$$\frac{\alpha_s(M)}{\pi} + \frac{9\mathbf{C}}{4} \left(\frac{\alpha_s(M)}{\pi} \right)^2 + \text{higher orders}$$

where \mathbf{C} is the Euler constant.

The perturbative expansion in Eq. (8) must be supplemented by the running formula for α_s , which for three active (massless) quarks takes the form [39]

$$\frac{\alpha_s(M)}{\pi} = \frac{4}{9} \left(\ln \frac{M^2}{\Lambda^2} \right)^{-1} - \frac{256}{729} \left(\ln \ln \frac{M^2}{\Lambda^2} \right) \left(\ln \frac{M^2}{\Lambda^2} \right)^{-2} + \dots \quad (9)$$

In the α_s^2 term in Eq. (8) it is legitimate to substitute the one-loop formula for α_s since the difference is of the higher order. (To make arithmetics simpler I used, however, the two-loop expression for α_s everywhere.) The expansion (9) is good only if $\ln(M/\Lambda)^2 \gg 1$. In our window this logarithm is not large, especially for higher values of Λ . Equation (9) is used literally, however; as an educated guess for the uncertainty I took the α_s^3 term in $I(M)$ and in the running law of the strong coupling constant.

In the domain of M where we are going to work the logarithm $\ln(M^2/\Lambda^2)$ is such that the two-loop terms in $I(M)$ and $\alpha_s(M)$ nearly compensate each other. Still the positive term $2.94(\alpha_s/\pi)^2$ in $I(M)$ is somewhat larger than the negative $1/\ln^2$ contribution coming from α_s/π . This means that the overall $1/\ln^2$ contribution to $I(M)$ is rather small and *positive*. The positivity implies that the fit of the sum rule with the two-loop accuracy will necessary produce a *lower* bound on the value of Λ than the fit performed with the one-loop formulae.

Let us turn now to the nonperturbative terms. The gluon condensate is defined as

$$\langle \mathcal{O}_G \rangle = \left\langle \frac{\alpha_s}{\pi} G_{\mu\nu}^a G_{\mu\nu}^a \right\rangle.$$

The coefficient C_G was calculated at one-loop order in [37],

$$C_G = \frac{\pi^2}{3}.$$

The two-loop answer is also known [40]. We will not need this two-loop result however. The correction is quite modest and is by far smaller than the existing uncertainty in the numerical value of the gluon condensate.

The four-quark condensate has a generic structure

$$\mathcal{O}_q = \bar{q}\Gamma q \bar{q}\Gamma q$$

where Γ stands for a combination of the Lorentz and color matrices. Two different combinations are relevant in the vector channel. I will not go into details since they are described at length in Ref. [37]. Anyway, the vacuum matrix element of \mathcal{O}_q is known only within the factorization hypothesis. Factorization becomes exact in the large N_c limit. Certainly, at $N_c = 3$ one could expect some deviations from factorization. For those operators which appear in the vector channel, the deviations are small, however. This fact was checked within the sum rules themselves, see e.g. [41], and on the lattices [42]. It seems quite safe to say that possible deviations from factorization are smaller than the uncertainty in $\langle \bar{q}q \rangle$. The anomalous dimension of

the operator \mathcal{O}_q is such, that it practically compensates the logarithmic dependence of $\alpha_s(M)$ appearing in C_q . Assembling all these elements together one gets [37]

$$C_q \langle \mathcal{O}_q \rangle = -\frac{448}{81} \alpha_s(\mu) \langle \bar{q}q(\mu) \rangle^2$$

where μ is a low normalization point, of the order of the typical hadronic scale, say ~ 700 MeV. If one uses the “standard” numerical values of the gluon and quark condensates the nonperturbative part of $I(M)$ takes the form

$$I(M)_{np} = 1 + 0.1 \left(\frac{0.6}{M^2} \right)^2 - 0.14 \left(\frac{0.6}{M^2} \right)^3 \quad (10)$$

where M is measured in GeV, the term M^{-4} is due to the gluon condensate, while the last term is due to the quark condensate. By the standard values I mean those accepted in Ref. [37]. The uncertainty in the gluon condensate is $\sim 30\%$ while in the four-quark condensate it can be as large as, perhaps, factor of 2.

If the gauge coupling constant is set to zero and quarks are allowed to propagate freely, then $I(M) = 1$. With the interaction switched on $I(M)$ deviates from unity, but the deviation is not large in the M interval shown.

The theoretical and experimental sides of the sum rule are compared on Fig. 2 which has been already discussed previously.

I pause here to make a remark concerning the truncation of the perturbative and power series in the theoretical side of the sum rule. As well-known, both series are asymptotic. The asymptotic nature of the α_s expansion in QCD was established long ago [43]. Recently it was proven [44] that the condensate series is also asymptotic. This means that including more and more terms of expansion in the theoretical prediction does not necessarily improves its accuracy. Since α_s is rather large in the domain of interest, $\alpha_s \sim 0.3$, it is clear that we must limit ourselves to just a few terms. A naive examination of Eq. (7) tells us that already the third term seems to fall into the tail of the asymptotic series which must be discarded. There is a fancy way to come to the same conclusion: summation of the so called renormalon chains. The renormalon chain is a sum of bubble contributions of any order in α_s presenting a subset of factorially divergent Feynman graphs. Since this is a very specific subset of all possible diagrams, I do not think that the renormalon chains are useful in the quantitative sense. The narrow choice of the set of graphs is not the main reason, however. The renormalons are inconsistent with the OPE-based approach, which is the only known basis for the proper treatment of QCD. Therefore, I would not say that including them improves our accuracy. They may serve, however, as an estimate of the uncertainty one can expect from theoretical formulae, in the absence of better ideas. Analysis of the renormalon chains in the context we are interested in, was carried out in Refs. [45] – [48] (as a matter of fact, some of these works were mainly devoted to the τ decays; the spectral densities in both cases are similar, and the conclusions are applicable to the sum rule (1) as well). And, sure

enough, the outcome of this rather sophisticated analysis boils down to saying that the uncertainty is of the order of the third term in Eq. (7).

For the same reasons inclusion of the power terms of high dimension seems to make no sense.

(iv) Sum rules in the $b\bar{b}$ channel

Voloshin's analysis [14], as it stands now, gives the most accurate evaluation of α_s , namely, $\alpha_s(M_Z) = 0.109 \pm 0.001$. The two-point function in the $b\bar{b}$ channel is calculated. The b quarks are treated in the nonrelativistic approximation. I would say that the work is very close in spirit to one of the recent lattice calculations [49] – the only distinction is that on the lattices the correlation function is calculated numerically (in order to set the scale in the physical units), while here we deal with an analytic calculation. Both approaches rely on experimental data in the $b\bar{b}$ channel. What is remarkable, is that the problems and difficulties one encounters are close in nature. For instance, higher-order relativistic corrections are disregarded in both cases. The same is valid for higher-order α_s corrections. Of course, the lattice analysis has its specific problems (especially with the light quarks) which are absent in analytic QCD. Since the stakes are very high it seems reasonable to be conservative. Making various *extreme* assumptions about possible effects due to higher-order relativistic and α_s corrections not accounted for in the sum rules [14] will probably allow one to stretch the error bars up to 0.003 or 0.004.

(iv) The lattice calculation

To complete the list of the Euclidean approaches used for determination of Λ or α_s let me mention the lattice calculations. The idea is straightforward – one fits the calculated spectrum in the $b\bar{b}$ or $c\bar{c}$ channels to the experimental one fixing in this way Λ_{lattice} in the physical units. Certainly, one has to overcome many technical problems and conceptual difficulties, especially with the dynamical light quarks. Having only quite a superficial idea about the lattice calculations I can not seriously comment on that. Fortunately, there is no need, since we had two beautiful talks at this Symposium [50, 51]. Details can be inferred from these talks. As far as I understand the main limitation is due to the fact that the full QCD lattice simulations have been carried out on a rather coarse lattice. In this sense the recent lattice revolution [52] turns out to be quite helpful. For instance, the NRQCD Collaboration uses the effective Lagrangian method to improve the coarse lattice spacing and extrapolates the results referring to $N_f = 0$ and $N_f = 2$ to $N_f = 3$. Using the one-loop perturbative matching for the plaquette action this group obtains [49] $\alpha_s(M_Z) = 0.115 \pm 0.003$. I suspect that the quoted error does not fully reflect the systematic uncertainties of the method. More specifically, I suspect that the one-loop expression which connects the measured plaquette action with the

gauge coupling constant may be less accurate than quoted above. If, say, the two-loop contribution is twice the square of the one-loop (and this does not seem crazy to me) one can easily get an extra 3% deviation in $\alpha_s(M_Z)$. Another possible source of uncertainty is the nonrelativistic approximation heavily exploited in Ref. [49]. Although the authors write: “it is clear that higher order relativistic corrections ... would be completely invisible” I am not quite confident that this is the case.

I should add a few words about other groups. The Kyoto-Tsukuba group uses the Wilson fermions to calculate the ρ meson mass and the charmonium energy levels [53]. The N_f extrapolation is also carried out, as well as that in the sea-quark mass. The result of the group is $\alpha_s(M_Z) = 0.111 \pm 0.005$. The conservative world average quoted in the review talk [15] is

$$\alpha_s(M_Z)_{lattice} = 0.112 \pm 0.007. \quad (11)$$

2.3 Problems with α_s from R_τ

My survey of the low-energy determinations of α_s would not be complete without discussing the total inclusive widths of the type $\tau \rightarrow \nu + \text{hadrons}$ or $\Upsilon \rightarrow \text{hadrons}$. Let me focus on the first process simply because it is much more often cited in the literature as a perfect source of information on α_s . One should keep in mind, however, that conceptually theoretical analysis of both processes is essentially identical.

As was noted above, the only low-energy “determination” of α_s which gravitates to the Z cluster rather than to the DIS one (i.e. yields a large value of $\alpha_s(M_Z)$) is that from R_τ . Experimentally, R_τ is measured to a rather good accuracy,

$$R_\tau = 3.65 \pm 0.05.$$

Braaten *et al.* suggested [54] to use this number in order to extract the value of $\alpha_s(M_\tau)$, which, being evolved up to M_Z , allegedly ensures high accuracy in $\alpha_s(M_Z)$, see Fig. 1.

One can not deny apparently appealing features of this proposal. Indeed, in this problem the state of the art analysis seems possible [54]. The perturbative corrections are known up to the third order in α_s . Mass corrections can be readily accounted for (they play little role, though). As far as the standard non-perturbative corrections are concerned, formally they start from the gluon condensate. However, one encounters here a fortuitous circumstance: accidental cancellation of the coefficient in front of $\langle \mathcal{O}_G \rangle M_\tau^{-4}$ in the leading (one-loop) order. This cancellation of the gluon condensate in R_τ is not backed up by any symmetries, and as a matter of fact, does not persist beyond the leading order. Since the gluon condensate appears only at the two-loop level, the corresponding contribution is numerically small. The four-quark condensate appears with the coefficient of the natural order of magnitude but is suppressed by M_τ^{-6} and is also small numerically. The combined effect of these two condensates is less than 2% [54]. If so, $\alpha_s(M_\tau)$ could seemingly be determined from the perturbative formula. Fitting the data for R_τ quoted above,

one gets $\alpha_s(M_\tau) = 0.36 \pm 0.03$ which results in a high value of $\alpha_s(M_Z)$ displayed on Fig. 1.

The R_τ determination of α_s was criticized in the literature more than once, see e.g. [45, 46, 47]. The aspect which was of greatest concern so far, is the truncation of the perturbative series, and its factorially divergent structure which shows up already in the third order since the coupling constant $\alpha_s(M_\tau) \sim 0.3$. Although this type of uncertainty is definitely present and affects the estimate of possible errors of the prediction, it seems unlikely that it alone is responsible for the value of α_s extracted from R_τ being too high. The renormalon chains are there irrespectively of whether we consider a process in the Euclidean or Minkowski domains. I think that the most dangerous theoretical contribution, which as a rule is not even mentioned, is specific to the processes with the essentially Minkowskian kinematics, like the hadronic width of τ . Conceptually, it is associated with the tail of the condensate (non-perturbative) series. This tail, manifesting itself in exponential terms (invisible in the truncated condensate series) is small if viewed at M_τ from the Euclidean side, but, unfortunately, is decaying much slower in the Minkowski domain and may be quite noticeable in the Minkowski analysis of R_τ carried out by Braaten *et al.* [54]. This asymmetric nature of the condensate series is a general property of QCD, it has nothing to do with the peculiarities of the τ decay.

In slightly different language the exponential contributions not controllable by the truncated condensate series are called *deviations from duality*. I can not dwell on this issue to the extent I would like to, the more so that it remains controversial. Still, some facts may be considered established [44]. In particular, it is known that although the deviations from duality are exponential in the Euclidean and in the Minkowski sides, the fall off is steep in the Euclidean domain and is much slower in the Minkowski domain. Moreover, in the Minkowski domain, the behavior of the exponential terms is *predicted* to be modulated by oscillations. It is very difficult, if possible at all, to make reliable predictions as to the specific law of the exponential fall off of the deviations from duality, the character of modulations, and the absolute value at given energy based on present-day QCD. One has to resort to phenomenological information (see Ref. [44] for further details).

Fortunately, we do have such information relevant to the τ decays. To substantiate my point let me show you Figure 6 presenting what I call “faked τ events”. This is the plot of the function

$$“R_\tau”/2 = 2 \int_0^{M^2} \frac{ds}{M^2} \left(1 - \frac{s}{M^2}\right)^2 \left(1 + 2\frac{s}{M^2}\right) [R(s)]_{e^+e^-}^{I=1} \quad (12)$$

calculated by S. Eidelman using the existing experimental data on $[R(s)]_{e^+e^-}^{I=1}$. The difference between the faked τ events and the actual decay is as follows. First, the spectral density used, refers only to the vector-vector correlation, while in the τ decays one deals with the vector-vector plus the axial-axial. Second, the integral runs up to M^2 where M is a free parameter. In R_τ we have the very same integral running up to M_τ ; needless to say that M_τ is fixed at its experimental value. The

weight function in Eq. (12) is the same as in the actual τ decays. The omission of the axial part of the spectral density plays no role in the aspect I want to highlight here.

I have indicated only one typical error bar, since the errors are 100% correlated. They are statistically uncorrelated in $[R(s)]_{e^+e^-}^{I=1}$ but are correlated in R_τ since R_τ is the integral over all points. The whole curve can be shifted up or down, by a standard deviation, as a whole, but the uncertainty in the relative position of the points is much smaller. This fact is of a paramount importance.

A single glance at Fig. 6 shows that the predicted oscillations do take place. It is clearly seen that at M_τ we are still *not* in the regime where one can use asymptotic formulae – i.e. truncated perturbative and condensate series. Indeed, at M_τ we are at the middle of the second oscillation which, presumably, must be followed by the third one while the asymptotic formulae are absolutely smooth and show no sign of oscillations. Of course, one can always close one’s eyes on the M^2 dependence and just fit one point on the curve at $M = M_\tau$ thus producing a “prediction” for $\alpha_s(M_\tau)$. The significance of this “prediction” is close to zero, however, since the theoretical contribution due to the exponential terms is not under control. One can speculate that the amplitude of the oscillations provides us with a measure of the theoretical uncertainty. If so, the error bar in $\alpha_s(M_\tau)$ must be doubled compared to the number quoted above, which means, in turn, that τ determination of α_s covers the whole interval from the low to the high values, and is not informative.

Today we have no theoretical method for estimating the size of the exponential contributions. It is quite clear, from this faked R_τ plot, that they are still noticeable at M_τ in the Minkowski domain. What can be said about $\Upsilon \rightarrow$ hadrons? The leading condensate correction in this process was found in Ref. [55] and turns out to be negligibly small. The condensate corrections, thus, are no menace to extracting α_s from this total width. The perturbative calculations of the ratio $\Gamma(\Upsilon \rightarrow \text{hadrons})/\Gamma(\Upsilon \rightarrow \mu^+\mu^-)$ were carried out in Ref. [56], in the next-to-leading approximation. The fit to Υ , Υ' and Υ'' yields [57] $\alpha_s(M_Z)_\Upsilon = 0.108 \pm 0.005$, where the error is dominated by the theoretical scale uncertainty in the perturbative formula, and all three resonances give consistent results. The experimental scatter is at the level 0.001 and can be neglected. However, *a priori* the analysis [57] is plagued by the same shortcomings as that of the τ decay: potentially uncontrollable exponential terms in the theoretical prediction for the width. On the positive side, the invariant mass of the hadronic state here is essentially higher than in τ , so that one may expect that the oscillations die off. On the other hand, the final hadronic state in the Υ decay is obtained from the gluon fragmentation, as opposed to the quark fragmentation in τ . It is known that deviations from duality are stronger in the gluon world than in the quark one [58]. Therefore, for the time being I would approach any results obtained from the total hadronic Υ width with extreme caution, even though they seemingly produce a low value of $\alpha_s(M_Z)$ which I advocate here.

3 If $\alpha_s(M_Z) \approx 0.11$ then ...

Summarizing the discussion in the previous part of the talk I conclude that $\Lambda_{\overline{\text{MS}}}^{(4)} \approx 200$ MeV follows from all determinations other than those at the Z peak. I hope that you are convinced now that the genuine value of $\alpha_s(M_Z)$ is close to 0.11, not to 0.125. Then we *have* to ascribe the apparent excess of the hadronic decays at the Z peak to new contributions unaccounted for in the Standard Model. First, I will consider immediate consequences for physics below 1 TeV and then comment on implications for Grand Unification.

3.1 New physics around the corner?

Assuming that the experimental number at the Z peak is correct we are forced to accept that some contributions due to new physics, invisible at low energies, show up at the Z peak. Although at the moment nobody can definitely say what kind of new physics will claim responsibility, it is curious to examine how the existing popular scenarios can cope with the situation.

The most developed scenario is a supersymmetric generalization of the Standard Model, the minimal supersymmetric standard model (MSSM). This model introduces a superpartner to every known particle, plus two Higgs superfields. Moreover, one usually assumes a very specific mechanism for the supersymmetry breaking: a breaking in the invisible sector through the gaugino condensation, transmitted to the visible sector only through gravity [59]. Then the masses of all gauginos turn out to be the same at the Planck scale. The soft mass parameters of all squarks and sleptons are set to be equal at this scale. This mechanism of the supersymmetry breaking is so popular among the SUSY model-builders that usually they do not differentiate between MSSM *per se* and the model supplemented by the above *additional* assumptions. Below this approach will be referred to as Constrained MSSM (CMSSM). I hasten to add that not a single experimental fact today points out to the existence of this particular mechanism of the supersymmetry breaking.

The next step crucially depends on the additional information we accept. Say, if we close our eyes on the $b\bar{b}$ excess and $c\bar{c}$ shortage at all, and rely only on the α_s clash, the opportunities for speculations are very vast. For instance, gluino and squarks in the 100 GeV ballpark could push up a little all the hadronic channels uniformly ($u\bar{u}$, $d\bar{d}$ and so on), so that the total excess of the hadronic width can approach the desired 10 MeV [60]. Another solution which might seem possible *a priori* is ultralight gluinos – so light that they change the law of running of α_s in the energy range of a few GeV [61]. It is clear, however, that in this way it is impossible to explain the ~ 10 MeV excess in the $b\bar{b}$ yield. Since this excess seems to be real, it is reasonable to try find a mechanism explaining simultaneously the $b\bar{b}$ problem and the α_s problem. As I mentioned in the beginning of the talk, relaxing the mass relations of CMSSM and arranging for a light stop and chargino helps do the job [11]. The general feeling, however, is that getting the 10 MeV excess in

the $b\bar{b}$ yield is not an easy exercise: it requires stretching the MSSM parameters to their extremes, so that the model is at the verge of contradicting the existing data (or, perhaps, even beyond this line [62]). Similar conclusions are achieved by other authors, as we heard today in Chankowski’s talk [63], although particular details are somewhat different. The general feeling is that the excess in the $b\bar{b}$ yield comes out too small in MSSM, smaller than the experimental number. Moreover, all experts share the opinion that this mechanism does not lead to any noticeable deviation in the $c\bar{c}$ yield compared to the Standard Model (see e.g. [3, 4, 11, 63]). Therefore, it looks like we must turn to alternative explanations.

In absolute numbers, the $b\bar{b}$ excess amounts to 11 ± 3 MeV while the shortage in the $c\bar{c}$ channel is 31 ± 13 MeV [18]. In the latter case, the relative experimental uncertainty is significantly larger than in the $b\bar{b}$ channel, so it is not crazy to assume that the effect will just evaporate with time. This “wait-and-see” attitude is perfectly reasonable and is accepted by many. On the other hand, it is also reasonable, and even tempting, to assume that some $c\bar{c}$ shortage does take place. The most attractive possibility is the assumption that the shortage in the $c\bar{c}$ channel is the same as the excess in the $b\bar{b}$ channel, say both are ~ 14 MeV. Combined with the information from α_s we can further speculate that the yield of *both* up quarks, u and c , is suppressed by 14 MeV, while the yield of *all* down quarks, d , s and b is enhanced by 14 MeV. Then the deviations from the standard model in the yields of the first and second generation quarks cancel in the total hadronic width. If t quark was light enough it would cancel the excess due to $b\bar{b}$. In the real world, however, this excess remains uncompensated.

Could such a pattern emerge in a natural way? The answer is yes, at least in principle, as is well-known to all those who played with four generations [64]. Indeed, let us assume that there exists a new weak-isospin doublet of quarks (or any other colored spinor fields), the masses of the *up* and *down* components of this doublet are heavier than M_Z , but not degenerate due to the spontaneous breaking of the $SU(2)_{weak}$ symmetry (let me call these components, symbolically, T and B). Then the diagram of Fig. 7 will produce the pattern of deviations exactly as described above. It goes without saying that in supersymmetric generalizations the graph of Fig. 7 has a tower of accompanying counterparts with sparticles in the loops.

Z can proceed into two gluons only because of axial coupling. Moreover, due to the Landau-Pomeranchuk-Yang selection rules both gluons then can not be simultaneously on the mass shell; one of the gluon propagators is contracted. The two-gluon intermediate state does not exist in this mechanism, a pleasant surprise by itself. As a matter of fact, the two-quark cut shown on Fig. 7 dominates since this contribution contains logarithm of m_T/m_B and is not suppressed by powers of $1/m_{T,B}$. The quark-antiquark-gluon intermediate state is suppressed by the second power of $1/m_{T,B}$ and can be neglected if $2m_{T,B} \gg M_Z$.

The logarithmic nature of the cut shown on Fig. 7 is pretty obvious; quantita-

tively one has [65]

$$\delta\Gamma(Z \rightarrow b\bar{b}) = 2 \left(\frac{\alpha_s}{\pi} \right)^2 \left(\ln \frac{m_B}{m_T} \right) \Gamma_A^{(0)}, \quad (13)$$

where $\Gamma_A^{(0)}$ is the parton probability of the axial Z decay into $b\bar{b}$ which amounts to $\sim (1/10)\Gamma(Z) \sim 240$ MeV. Since $\delta\Gamma$ is due to interference between the amplitude $Z \rightarrow 2g \rightarrow q\bar{q}$ (i.e. $I = 0$) with the direct $Zq\bar{q}$ coupling ($I = 1$), the sign of $\delta\Gamma$ is different for $q = u, c$ on one hand, and $q = d, s, b$ on the other. Thus, the shortage of $c\bar{c}$ is automatically the same as the excess of $b\bar{b}$. The formula (13) is obtained with the standard quarks, doublets with respect to $SU(2)_{weak}$ and triplets with respect to $SU(3)_{color}$. If the logarithm is of order one, the effect is way too small to be important, of the order of 1 MeV.

In principle, it is not so difficult to enhance it by an order of magnitude by saying that B and T quarks are color octets or weak isotriplets, and so on in the same vein. Moreover, in supergeneralizations it may well happen that the tower of graphs with sparticles in the loops add up coherently. This is not a problem. The true problem is avoiding spoiling the successful predictions of the standard model, say, the ratio of M_Z/M_W . Experts say, that new isodoublets contributing so significantly into the imaginary part will shift the ratio M_Z/M_W beyond the allowed limits [66]. The assertion [66] refers, however, only to the standard heavy quarks, replicas of the existing one. Whether it is still valid for color octets or isotriplets, or a mixture of a little bit of everything, is not clear to me at the moment.

Of course, this mechanism is rather *ad hoc*; moreover, it goes against the existing trend (or deep belief, if you wish) that any new physics has to be associated exclusively with MSSM. Well, at this stage it seems reasonable to keep our eyes open, making no ultimate commitments based only on theoretical prejudice or shaky arguments. After all, in the past Nature surprised us more than once.

It is worth noting that the induced $I = 0$ vertex $Z\bar{q}q$ discussed above will also change all predictions for asymmetries and sine-squared of the Weinberg angle. If the induced $Zb\bar{b}$ coupling is purely axial the corresponding change in the asymmetry \mathcal{A}_b is too small to be important. If, however, the “new physics” contribution to $Zb\bar{b}$ involves predominantly the right-handed b quarks (as may be the case in some supersymmetric scenarios) then the induced $Zb\bar{b}$ coupling noticeably changes the prediction for \mathcal{A}_b . Adjusting the coupling in such a way as to reproduce $\delta\Gamma(Z \rightarrow b\bar{b}) \sim 10$ MeV we simultaneously lower \mathcal{A}_b by $\sim 8\%$ compared to the SM predictions [67]. Remarkably, the value of \mathcal{A}_b detected at SLC is $\sim 8\%$ lower than the SM value ($\sim 2\sigma$ effect) [18]. The supersymmetric extension of the diagram of Fig. 7, with two gluinos in the intermediate state, may produce the $I = 0$ $Z\bar{q}_R q_R$ vertex provided that the right-handed squark is essentially lighter than the left-handed one. It is curious that if the diagram of Fig. 7 vanishes with switching off the $SU(2)$ symmetry breaking, its supergeneralization need not vanish in this limit (although it vanishes, of course, with switching off the supersymmetry breaking). This is due to the Z

boson coupling to the weak hypercharge. Therefore, these graphs may bring in a new scale. There are many open questions, though:

- can the induced $Z\bar{q}_R q_R$ vertex contain enhancement factors of order 10 compared to the natural scale $(\alpha_s/\pi)^2$?
- if they do appear does this scenario go through the successful predictions of the standard model?

Further detailed analysis is clearly in order.

3.2 Grand Unification

Now I proceed to the second aspect of the small-versus-large α_s problem – Grand Unification. It is difficult for me to go into details here not only because this topic is vast and I have very little time left, but also because I am a newcomer in this field and still have to learn a lot. I see experts in this audience who will definitely elaborate the point in their talks.

The idea of Grand Unification is extremely attractive. I am aware of no other explanation of the fact that the electric charge is quantized [68]. At the same time I feel rather uneasy, since Grand Unification requires extrapolating the theory from the known range of 100 GeV up to 10^{16} GeV, fourteen orders of magnitude. I am a down-to-earth person and would like to stay away from speculations as to what is going to happen with the theory on the way to 10^{16} GeV. Still one most naive assumption – essentially nothing happens – is worth considering, say, for the purpose of getting a proper reference point.

The plots presented below (Fig. 8), which I borrowed from Langacker [69], are well known. The first one, quite famous a couple years ago, shows that in the Standard Model the evolution lines of three coupling constants, α_1 , α_2 and α_3 do not intersect in one and the same point, so that the naive straightforward unification does not go through in SM. The second plot is meant to be a triumph of the naive unification within MSSM; it is designed to illustrate that the three evolution lines perfectly intersect in the naive Constrained MSSM (i.e. with no GUT threshold corrections and/or non-renormalizable operators (NRO) [70] from the Planck scale physics added). The only thing which remains to be added is the value of $\alpha_s(M_Z)$ ensuring this intersection. This value jiggles a little in different publications, depending on how the SUSY threshold effects are treated, and what corridor for the Weinberg angle is accepted, but it jiggles between 0.125 and 0.128! Alas, the “triumph” of the CMSSM Grand Unification is rather to be called a failure today.

Does this mean that the Grand Unification scenario is ruled out? Certainly not. There exist three obvious ways out:

- (i) three gauge coupling constants need not intersect exactly at one point if GUT threshold corrections and/or Planck scale NRO’s play a role;
- (ii) the mechanism of SUSY breaking inherent to CMSSM can be traded for another mechanism where specific relations between the sparticle masses appearing

in CMSSM do not hold;

(iii) new physical phenomena can take place at an intermediate scale, half way between the present-day 100 GeV and the GUT scale.

Let me comment on these three possibilities in turn. The first option seems rather obvious and natural. One definitely expects some effects due to the fact that not all superheavy particles have exactly the same mass. The size of the effects due to the GUT scale thresholds (or NRO) has been investigated more than once (for recent analyses see Ref. [71, 72]). In many instances they come out positive, i.e. lead to larger values of α_s , thus only aggravating the problem. Even if they can be made negative the size of these corrections is typically not large enough. The interval of $\alpha_s(M_Z)$ emerging from the supersymmetric GUT is believed to be [73] 0.12 to 0.14, with the typical prediction lying around 0.128. I hasten to add, though, that other authors [72] seem to be able to get down almost to 0.11. Still, my feeling is that with a natural set of assumptions it is difficult to descend down to 0.11. So what?

The strategy which seems more adequate to the present stage is as follows: let us assume that $\alpha_s(M_Z) = 0.11$ and find out what can be said then about the GUT thresholds, and possible observable implications at low energies. This strategy is pursued by Lucas and Raby [6]. Based on an overall analysis of relevant operators in the SO(10)-based grand unification, compatible with phenomenological pattern of masses and angles, these authors find that $\alpha_s(M_Z) = 0.11$ perfectly fits provided a certain relation between the masses of the superheavy Higgses is accepted. It is remarkable that this relation implies a significantly lower prediction for the proton lifetime compared to the one emerging in the naive constrained MSSM. The Lucas-Raby result for the proton lifetime is much closer to the values accessible to experiment. This makes the issue of the proton decay exciting again, extracting it from oblivion it resided in during the last few years when people believed that the theoretical expectation is so high there is no hope of detecting the proton decay experimentally in the foreseeable future.

The second route to explore is relaxing some of the assumptions of CMSSM, in particular the mechanism of the SUSY breaking. If the SUSY breaking is transferred only through gravity [59] the masses of all gauginos are the same at the GUT scale. This implies that at our scale gluino is ~ 3 times heavier than the wino. Other mechanisms of the SUSY breaking were under discussion in the mid-eighties. In particular, the one which seems promising is the instanton-generated dynamical breaking suggested by Affleck *et al.* and Veneziano *et al.* [74]. Theoretically it is an absolutely beautiful mechanism. Unfortunately, no scheme which was phenomenologically nice and attractive was found in the eighties, and the whole idea has been in a dormant state since then in connection with the rise of the gravity-induced scheme of Chammseddine *et al.* mentioned above. Recently, however, the instanton ideas were revived by Dine *et al.* [75], who submerged into the search for a phenomenologically successful and aesthetically attractive model with vigor. It is not ruled out that such a model will be found. Since this mechanism is based on the SUSY

breaking at a relatively low scale, no relation between the gluino and wino masses emerges, generally speaking. Anticipating the success of the search one could ask the question: what happens with the prediction for $\alpha_s(M_Z)$ from the naive Grand Unification (no GUT thresholds and so on) if the relation between the gluino and wino masses is relaxed?

The answer to this question was given recently by Roszkowski and myself [10]. As a matter of fact, the result for $\alpha_s(M_Z)$ is essentially independent on all details except the gluino and gaugino masses. It is not difficult to get $\alpha_s(M_Z) \approx 0.11$, provided that the gluino is relatively light (100 to 200 GeV) while the wino is relatively heavy (600 GeV and heavier). Of course, the precise number requires full calculations (which were performed) but the tendency becomes immediately clear upon reflection about what happens with the coefficients in the Gell-Mann-Low functions when one freezes out this or that particle. It is obvious that making gluino light helps, as well as making the wino heavy. Roughly, the ratio of the corresponding masses needed is 1/3, i.e. inverse compared to what one gets in CMSSM (see Fig. 9).

Finally, the third option – new physics, other than MSSM, at an intermediate scale – is advocated, for instance, in Refs. [7, 8]. As you could see from the previous section, I absolutely do not rule out and, on the contrary, to an extent expect the advent of “new” new physics in the great desert predicted by MSSM in the energy range from $\sim 10^2$ to $\sim 10^{16}$ GeV. This is a viable option, but it is very difficult to pursue this scenario, since we have very little evidence to assume something definite, and, hence, very little predictive power. The same refers to non-renormalizable operators coming from the Planck scale – string physics. They definitely could enter the game [76], but since very little is known about physics at this scale, and too much depends on pure speculation, I prefer to refrain from going into this topic, leaving the field completely open to experts.

Summarizing, the issue of Grand Unification has made a full turn now; we are back to square one. While the most naive version, with no new effects (apart from superpartners) in the interval from 100 GeV up to the GUT scale, produces too high a value of $\alpha_s(M_Z)$ refinements can, in principle, bring this value down to 0.11. The question is what refinements are to be done. It seems that additional information (or divine guidance) is needed in order to answer the question.

4 Conclusions

I still remember that in 1970, before the QCD era and before the Standard Model, some people discussed $K_L - K_S$ mass difference in the most naive manner, calculating diagrams with free quarks, and concluding that the small value of this mass difference can be explained in no other way than a new particle, charmed quark, compensating for a large contribution coming from the virtual u quark. Many were very skeptical, since these calculations were perceived as far too naive [77], and, moreover, this was just one number, one contradiction with an analysis which did not smell too

clean. So, they preferred to wait. We all know now who was right and who was wrong. I think that now Nature gives us a very similar sign, perhaps the only one we can get from it with the existing machines. It would be unforgivable to loose this chance. Since 1974, we have waited for a miracle, for the advent of new physics. And now, when it seems already with us, we must keep our eyes open to it. If we are optimistic enough, we should say that new physics is already discovered and try to extract maximal information from this fact. For those who are more cautious I can only repeat that (i) the α_s testimony can not be overruled, and (ii) we definitely have now at our disposal *three or four* phenomena where 2 to 3σ deviations from SM are detected. These observations are uncorrelated experimentally, therefore the probability of statistical fluctuations must be multiplied, which leaves us with a pretty improbable interpretation, at the level of 10^{-4} or less, unless we invoke new physics.

Finally, the minimal lesson refers to the QCD practitioners. It became fashionable in many works devoted to the low-energy hadronic physics, to use, as the most precise value of α_s the one stemming from the determination at the Z peak. The 13% shift in α_s at high energies in many instances produces quite a dramatic effect at low energies. I would like to urge not to follow this fashion blindly. The determination from the Z peak is *not* the most precise one, for the reasons I have explained. The low-energy calculations should use the value of α_s extracted from the low-energy processes, which is inconsistent with that determined at the Z .

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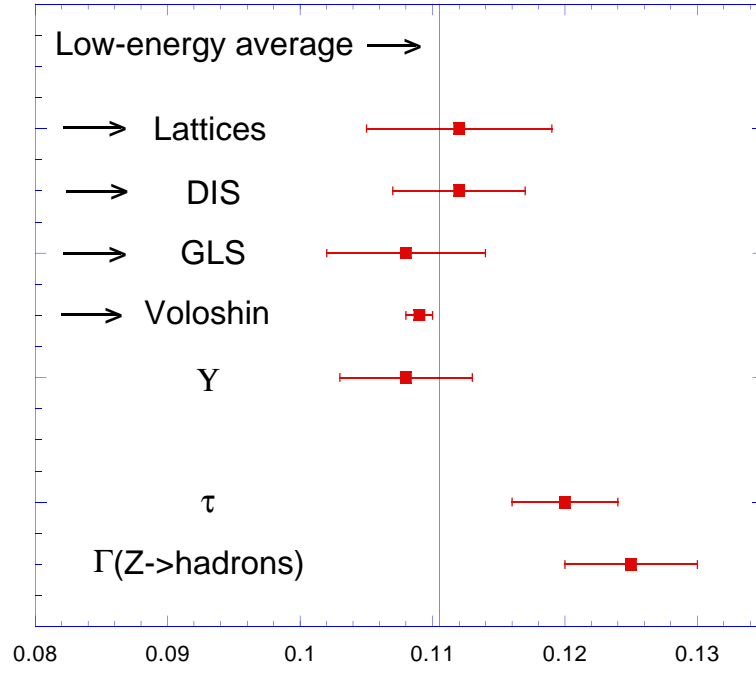


Fig. 1. Experimental data on $\alpha_s(M_Z)$. (Udapted from Ref. [13]). The vertical line is a naive average of four low-energy points marked with the arrows.

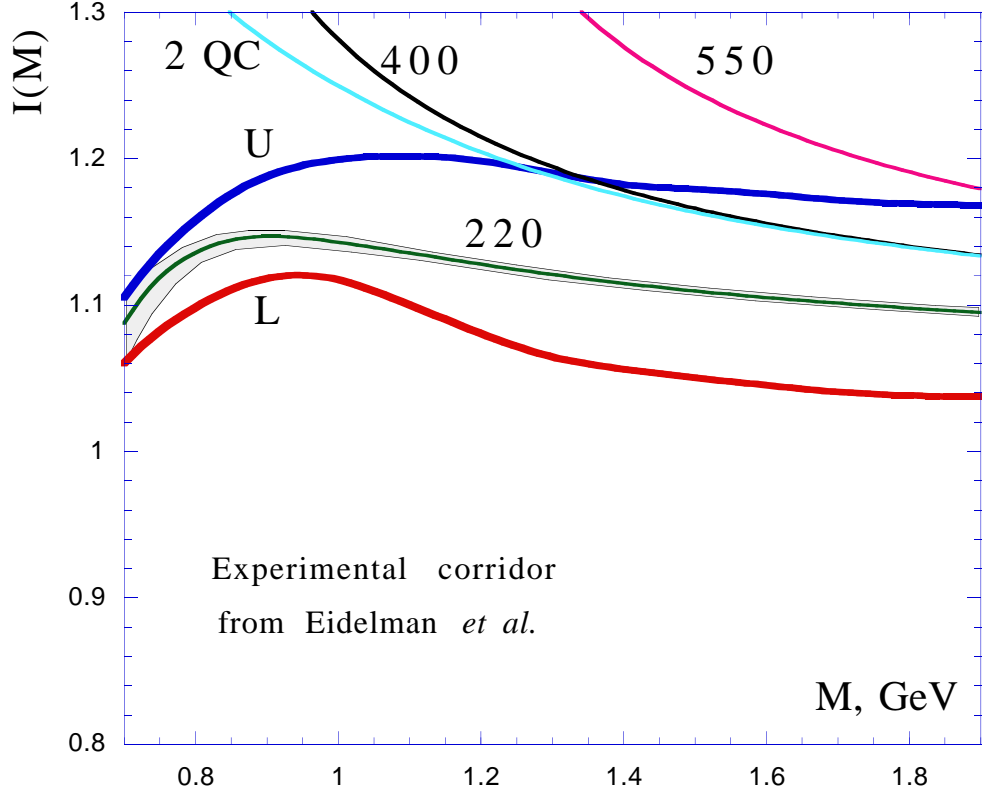


Fig. 2. The QCD sum rule in the ρ meson channel for different values of the scale parameter $\Lambda_{\overline{\text{MS}}}^{(3)}$. The curves marked by “U” and “L” are the upper and lower sides of the experimental corridor (see text and Ref. [16]). The curve marked by “2 QC” corresponds to $\Lambda_{\overline{\text{MS}}}^{(3)} = 400$ MeV and the doubled value of the quark condensate.

Fig. 3. Determining the strong coupling constant from deep inelastic scattering (from Ref. [29]).

Fig. 3a. Next-to-leading order QCD fit to the deuterium structure function measured by SLAC and BCDMS in the central x domain. The solid line is the result of the fit; the dashed line visualizes the Q^2 evolution without the higher twist effects (the target mass corrections are included, however).

Fig. 3b. The logarithmic derivative $d \ln F_2(x, Q^2)/d \ln Q^2$ at a high value of Q^2 . The solid curve corresponds to $\alpha_s(M_Z) = 0.113$. The dashed curves correspond to 0.123 and 0.103.

Fig. 4. The variation of $\alpha_s(M_Z)$ as a function of a scale parameter k (from Ref. [29]).

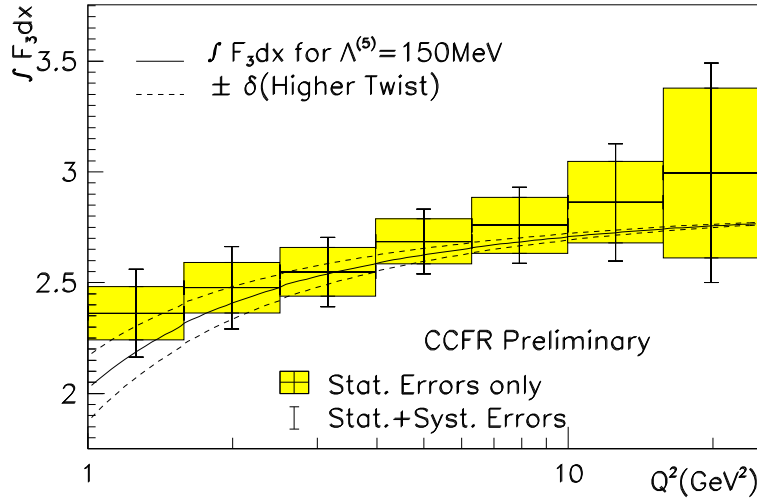


Fig. 5. The QCD fit to the Gross-Llewellyn Smith sum rule. The solid line is the result of the fit corresponding to $\alpha_s(M_Z) = 0.112$ (from Ref. [36]).

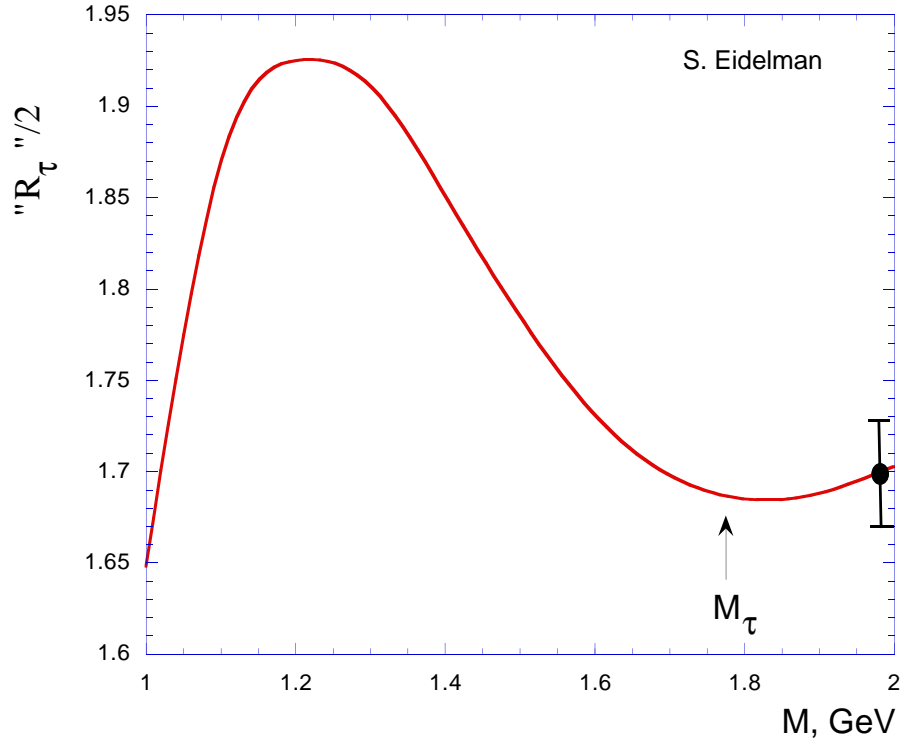


Fig. 6. Eidelman's fake τ events versus the faked τ mass (see text). The curve can be shifted as a whole up or down within the limits indicated by the error bar on the right-hand side.

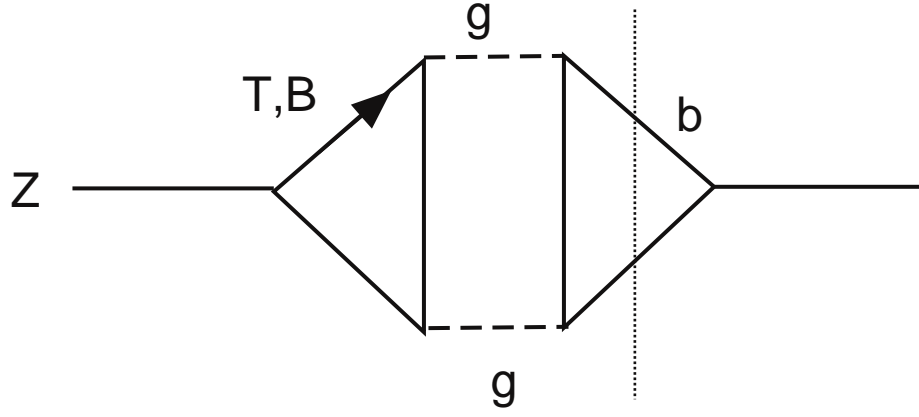


Fig. 7. The two-loop graph giving rise to $\delta\Gamma(Z \rightarrow \bar{b}b)$. The $\bar{b}b$ cut is indicated by the dotted line.

Fig. 8. The evolution of the coupling constants within the Standard Model and MSSM (from Ref. [69]).

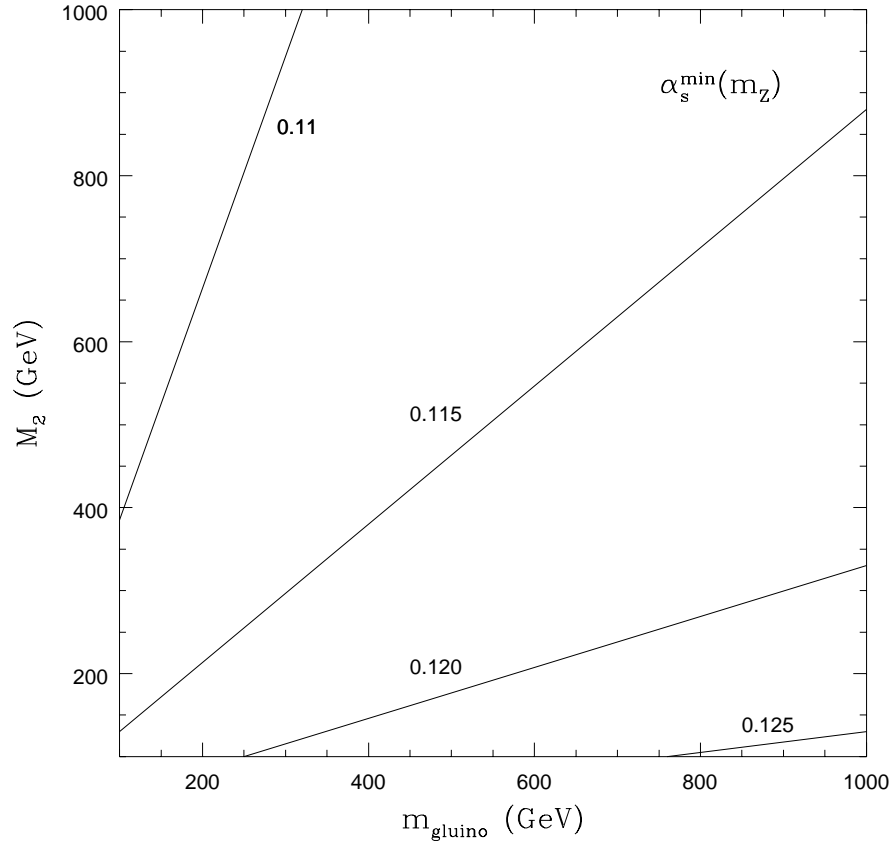


Fig. 9. Contours of constant $\alpha_s(M_Z)$ in the plane $(m_{\tilde{g}}, M_2)$ (from Ref. [10]). All other parameters are chosen in such a way as to minimize $\alpha_s(M_Z)$. The upper left corner of the plot presents a phenomenologically acceptable domain.

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